



A D D I T I O N S
T O T H E
History and Present State
O F
E L E C T R I C I T Y,
W I T H
O R I G I N A L E X P E R I M E N T S.

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T H E S E C O N D E D I T I O N.

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T H E
P R E F A C E.

THE method I took to distinguish the books I had seen from those I had not seen, in *the catalogue of electrical authors*, subjoined to the History of Electricity, has been attended with the advantage I promised myself from it. Several persons, who were in possession of books I had not seen, have communicated them to me, and I have carefully perused them, and digested their contents into the second edition, and printed them separately for the benefit of those who purchased the first. Far the greater part of these new authors, the reader will perceive by the catalogue, were German, and wrote in High Dutch, a language with which, I believe, the *literati* of this country are but little acquainted, which might be the reason why neither myself nor my friends had ever heard of them before. Though the new materials they have supplied cannot be said to be of the first importance, many of the articles are very curious, and I hope the reader, as well as myself, will think that they have well repaid me for my trouble in learning the language.

It is certainly much to be regretted, that philosophers have not one common language; but neither the theory of language in general, nor the nature and analogies of things to be expressed by it are yet sufficiently understood, to enable us to contrive a new and philosophical one, that might be easily learnt, and would be completely adequate to all the purposes of science; and Latin is a language which persons of a philosophical turn of mind have seldom leisure to make themselves so much masters of, as to enable them to write in it with that elegance which the taste of the age requires. Besides, books written in Latin are but little read, at least in England, and therefore could have no sale with us. These circumstances make it the more necessary, that there should be, in every country, persons possessed of a competent knowledge of foreign languages, who should be attentive to the progress of science abroad, and communicate to their countrymen all useful discoveries as they are made.

BESIDES additions to the history, and other parts of the work, the reader will find three entire new sections of original experiments.

LEEDS,
JANUARY, 1769.

ADDI-

A D D I T I O N S

T O T H E

HISTORY of ELECTRICITY.

PAGE 2. MR. BOZE is said to have shown, that the *agate* was known very early to have electric powers.*

5. ABOUT thirty years after Gilbert, Nicolaus Cabæus, a Jesuit of Ferrara, repeated his experiments; and found that *white wax*, almost all the *gums*, and *crude gypsum*, were to be ranked among electric bodies.†

7. MR. BOYLE got a glimpse, as we may say, of the electric light, for he found that a curious diamond, which Mr. Clayton brought from Italy, gave light in the dark, when it was rubbed against any kind of stuff; and he found that, by the same treatment, it became electrical. He also observed the same property in several other diamonds.‡

* Geschichte, Vol. I. p. 179.

† Ib. p. 180.

‡ Secondat's History of Electricity, p. 141.

9. THE members of the academy at Florence, about the time of Otto Guericke, made some electrical experiments with amber and gems. They observed that *flame* was not affected by electricity; but that it attracted all fluid bodies, and among the rest quicksilver; and that electric bodies showed no power when they were rubbed by smooth bodies, as glass, crystal, or ivory.*

46. MR. DU FAYE found that not only damp air, but also great heat was prejudicial to electricity, and that his experiments often failed in the warmest hours of a moderately hot day.†

Ib. IN order to determine (as he imagined) whether some bodies did not contain more electric matter than others, he hung upon an iron bar, suspended on silken strings, threads of silk, wool, cotton and linen; and observed that when the excited tube was brought near the bar, the linen threads separated to the greatest distance, and so were most affected; the cotton next, then the silk, and the wool least of all, the threads hanging almost close together. From this he concluded, that of all these substances, linen contained the most electricity in it.‡

48. FROM his experiments concerning the electric spark drawn from animal bodies, he, at that time, concluded, that the bodies of living animals (and also metals) were surrounded with an atmosphere of vapour, which was actually set on fire by the electric light.§

Ib. HE observed, that a cat emitted an electric spark, which evidently gave her pain, when the finger was brought to any

* Geschichte, Vol. 1. p. 182.

† Ib. Vol. 1. p. 211.

‡ Ib. Vol. 1. p. 231.

§ Ib. Vol. 1. p. 215, 230.

part of her body, after she had been stroked, while she was sitting on a silk cushion. This must have appeared very extraordinary, before it was known, that the electric matter passed from the hand to the cat, in the act of rubbing.*

48. WITH the electric spark, he imagined he could have fired inflammable substances; and he made several attempts with tinder and gunpowder, but without success: He found no appearance of real fire. This was a capital discovery reserved for the Germans.†

63. MR. FAYE, in the memoirs of the academy of sciences for the year 1737, acknowledges that the experiments of Mr. Grey and Mr. Mortimer, concerning the revolutions of light bodies round an electrified ball, did not succeed with him. But says, they were the only ones of Mr. Grey's which had not; and, with a temper becoming a philosopher, adds, that he doth not, therefore, say, that they never had succeeded; but seems rather willing to attribute the failure with himself, to his omitting some circumstance, not mentioned by those gentlemen, though, unknown to them, it might be of principal consequence to the experiment.‡

69. THE prize gained by Dr. Desaguliers was a medal of the value of 300 livres, proposed, at the request of Mr. Harpez de la Force, for the best essay on electricity, and shows how much this subject engaged the attention of philosophers at that time.§

70. ACCORDING to some accounts, Christian Augustus Haufen, professor of mathematics at Leipzig, was the first who re-

* Geschichte, Vol. 1. p. 216.

† Ib. Vol. 1. p. 229.

‡ Ib. Vol. 1. p. 226.

§ Ib. Vol. 1. p. 261.

vived the use of Hawkeſbee's glaſs globe, and Mr. Boze, who was excited to make experiments in electricity by the example of Mr. Hauſen, borrowed this capital improvement from him.*

72. MR. WINCKLER contrived to rub glaſs, and china veſſels, in the inſide; and he ſays they acted as ſtrongly on bodies placed on the outſide of them, as when they were rubbed on the outſide.†

73. MR. GORDON obſerved that electrified water evaporated faſter than water not electrified, when expoſed in ſimilar glaſs veſſels.‡

Ib. HE produced ſtrong electric ſparks by conveying electricity with iron wires, to the diſtance of 200 ells from the place of excitation. He alſo found that the ſparks were ſtronger when the wire was thick than when it was ſmall.§

Ib. MR. WAITZ made his glaſs tubes act ſtronger by rubbing them with a waxed cloth and a little oil. He alſo found, that glaſs made with very little potaſh, acted much better than that in the compoſition of which much of it was uſed; but it required longer time, and more heat to vitrify it. He got ſome glaſs made on purpoſe to aſcertain the fact.||

Ib. BY various experiments of attraction and repulſion, which Mr. Waitz made in rubbing a dog (which he had made thoroughly dry for that purpoſe) he proved that the ſlaſhes of light, which ſometimes appear when animals are ſtoked, are electrical. This had been ſuppoſed, but was not accurately aſcertained before.** It was this gentleman who gained the prize of 50 ducats, propoſed in the year 1744, by the academy

* Geſchichte, Vol. 1. p. 278, 279.
p. 317.

§ Ib. Vol. 2. p. 359.

† Ib. Vol. 2. p. 400.

|| Ib. Vol. 2. p. 381.

‡ Ib. Vol. 2.

** Ib. p. 385.

of sciences at Berlin, for the best dissertation on the subject of electricity. It was published along with three others, which were offered at the same time, and thought worthy of that honour.*

74. MR. LUDOLF the younger, demonstrated, that the luminous barometer is made perfectly electrical by the motion of the quicksilver; first attracting, and then repelling bits of paper, &c. suspended by the side of the tube, when it was inclosed in another, out of which the air was extracted.† Before this experiment, these effects had been ascribed to the air.

Ib. PROFESSOR HAMBERGER and Mr. Waitz had discovered that the motion of quicksilver in a glass vessel, out of which the air was extracted, had the power of moving light bodies; and Mr. Allamand likewise found, that it made no difference whether the vessel had air in it or not.‡

75. THE electrical star was contrived by Mr. Gordon; and by turning the points a little obliquely, he was surprized to find it began to move of itself.§

Ib. MR. GOTTFRIED HEINRICK GRUMMERT, of Biala in Poland, made a curious experiment upon electric light, which, as we shall see, was afterwards made and pursued to great advantage by Dr. Watson and Mr. Canton. In order to observe whether an exhausted tube would give light when it was electrified, as well as when it was excited, he presented one, eight inches long, and a third of an inch wide, to the electrified conductor, and was surprized to find the light dart very vividly the whole length of the tube. He also observed,

* Geschichte, Vol. 2. p. 380.

† Ib. Vol. 3. p. 495.

‡ Ib. Vol. 2. p. 426.

§ Ib. Vol. 2. p. 317, 358.

that, some time after the tube had been presented to the conductor, and exposed to nothing but the air, it gave light again, without being brought to any electrified body. This light in *vacuo* Mr. Grummert proposed to make use of in mines, and places where common fire, and other lights cannot be had, and for this purpose, he mentions several methods of increasing this light.*

I SHALL conclude this account of the discoveries of the German philosophers in this period, with a very curious one of Professor Kruger, concerning the change made on the colour of bodies by the electric effluvia. In order to try whether there was any thing of sulphur in these effluvia, he exposed the red leaves of wild poppies to the electric spark, and found that they were presently changed to white. He was not able to produce any change in yellow colours, nor in blue immediately; but found that when they had lain a day or two, after being exposed to this operation, they became white. In these experiments the leaves were fastened with white wax to plates of tin.†

75. SUCH a general attention was excited to electricity by the curious discoveries of the Germans, that, in the year 1745, electrical experiments were exhibited, in Germany and Holland, for money, as a show; and public advertisements appeared in the news-papers for that purpose.‡

82. THE person who first made the great discovery of the electric shock, was Mr. Von Kleist, Dean of the cathedral in Camin; who, on the 4th of November 1745, sent an account of it to Dr. Lieberkuhn at Berlin. This account, as taken by

* Geschichte, Vol. 1. p. 417.

† Ib. Vol. 3. p. 493.

‡ Ib. Vol. 2. p. 399.

Mr. Galath out of the register of the academy at Berlin, to which it had been communicated, is as follows. “ When a
“ nail or a piece of thick brass wire, &c. is put into a small
“ apothecary’s phial, and electrified, remarkable effects follow:
“ but the phial must be very dry or warm. I commonly rub
“ it over beforehand with a finger, on which I put some
“ pounded chalk. If a little mercury, or a few drops of spirit
“ of wine, be put into it, the experiment succeeds the better.
“ As soon as this phial and nail are removed from the electri-
“ fying glass, or the prime conductor, to which it hath been
“ exposed, is taken away, it throws out a pencil of flame so
“ long, that, with this burning machine in my hand, I have
“ taken above sixty steps, in walking about my room. When
“ it is electrified strongly, I can take it into another room, and
“ there fire spirits of wine with it. If, while it is electrifying,
“ I put my finger, or a piece of gold, which I hold in my
“ hand, to the nail, I receive a shock, which stuns my arms
“ and shoulders.

“ A tin tube, or a man, placed upon electrics, is electrified
“ much stronger by this means than in the common way.
“ When I present this phial and nail to a tin tube, which I
“ have fifteen feet long, nothing but experience can make a
“ person believe how strongly it is electrified. I am per-
“ suaded, that in this manner, Mr. Boze would not have ta-
“ ken a second electrical kiss. Two thin glasses have been
“ broken by the shock of it. It appears to me extraordinary,
“ that when this phial and nail are in contact with either con-
“ ducting or non-conducting matter, this strong shock does
“ not follow. I have cemented it to wood, metal, glass, sea-
“ ling-wax, &c. when I have electrified without any great
“ effect. The human body, therefore, must contribute some-
“ thing

“ thing to it. This opinion is confirmed by my observing,
 “ that, unless I hold the phial in my hand, I cannot fire spi-
 “ rits of wine with it.”*

NOTWITHSTANDING Mr. Kleist immediately communicated an account of this famous experiment (which indeed, it is evident, he has but imperfectly described) to Mr. Winckler at Leipfick, Mr. Swietliki of Dantzick, Mr. Kruger of Hale, and to the professors of the academy of Lignitz, as well as to Dr. Lieberkuhn of Berlin abovementioned, they all returned him word, that the experiment did not succeed with them. Mr. Gralath of Dantzick was the first with whom it answered; but this was not till after several fruitless trials, and receiving farther instructions from the inventor.†.

THE Abbé Nollet had information of this discovery, and in consequence of it, says, in a letter to Mr. Samuel Wolfe, of the society of Dantzic, dated March 9th 1746, that the experiment at Leyden was upon principles similar to this made with a phial half full of water, and a nail dipped in it; and that this discovery would have been called the Dantzic experiment, if it had not happened to have got the name of that of Leyden.‡

82. Mr. ALLAMAND and Mr. Muschenbroeck were the first who repeated and published an account of the experiment of the phial, in Holland, the Abbé Nollet and Mr. Monnier in France, and Mess. Gralath and Rugger in Germany.§

85. Mr. GRALATH made the shock much stronger by using a glass vessel five inches in diameter, with a narrow neck, ten inches long; by substituting an iron wire with a knob of

* Geschichte, Vol. 1. p. 407.

† Ib. p. 411.

‡ Ib. p. 409.

§ Ib. Vol. 2. p. 433.

tin for the iron nail, and water for spirit of wine*. He first found, that the same shock could be communicated to a number of persons, who took hold of one another's hands; if the person at one extremity of the line they made, touched the outside of the phial, and he at the other touched a wire communicating with the inside. In this manner, on the 10th of April 1746, he gave a shock to twenty persons; and he says, he did not doubt, but it might be given to a thousand.† When these persons were connected by pieces of metal, and did not hold one another's hands, they found the same shock, but not when they held wood, and other imperfect conductors.‡ This gentleman, also, gave a shock, by means of long wires, to a person standing in a garden, while he himself, who directed the experiment, had the machine in a part of the house at a considerable distance. And he was the first who made what we now call an *electrical battery*; for he increased the shock by charging several phials at the same time.§ Lastly, he observed, that if the phial had the least crack in it, it could never give a shock; and also that when a phial was discharged, it acquired a small charge by standing, without receiving any thing from the machine, so as to give a small shock.|| This is what we now call the *residuum* of a charge, and is properly part of the charge that lay on the uncoated part of the phial, which doth not let go all its electricity at once; so that it is, afterwards, gradually diffused to the coating.

85. MR. WINCKLER found out the method of making the discharge of the phial without feeling the shock himself, by not bringing his own body into the direct circuit.**

* Geschichte, Vol. 2. p. 411. † Ib. p. 439.

‡ Ib. p. 440. § Ib. p. 552.

|| Dantzick Memoirs, Vol. 1. p. 514, 516.

** Geschichte, Vol. 2. p. 459.

85. HE also gave the shock when several ells of running water, in his garden, made part of the circuit. This he did on the 28th of July 1746, about the same time that Mr. Monnier performed the same experiment in France.* And, lastly, he found that the more globes were used, and the larger they were, the stronger was the shock.† This must be the consequence of increasing the power of excitation.

85. WHEN Mr. Gallabert made the Leyden experiment with hot water, the phial broke by a spontaneous discharge, and a circular piece, two lines and a half diameter, was thrown from the place of the rupture, against a wall, which was at the distance of five feet. The vessel, he says, had no crack, or other injury.‡

95. BOTH Mr. Gralath and Mr. Richman observed, in several cases, a stronger spark between two bodies when both of them were electrified, than when only one of them was in that state; but neither of them suspected, that the electricities were of a different kind.§

101. MR. MONNIER, who made the experiment of transmitting the electric shock through a basin of water in the Thuilleries, mentioning the quantity of surface in the basin, as if it was of consequence to the experiment, and expressly saying it was *electrified*; Mr. Gralath made several experiments, which prove, that bodies which form the circuit of the shock are not properly electrified.||

133. MR. GRALATH constructed an electrometer upon the same principle with that of Mr. Ellicott.**

* Geschichte, Vol. 3. p. 504. † Ib. p. 526. ‡ Jallabert's Experiences, p. 128.

§ Wilcke's Preface to the German Version of Franklin's Letters. || Geschichte, Vol. 3. p. 552. ** Dantzick Memoirs, Vol. 1. p. 525.

171. MR. WINCKLER enumerated many particulars, in which electricity and lightning resemble one another.*

172. It was a small step towards discovering the effect of pointed bodies, that Carolus Augustus van Bergen, professor of medicine at Frankfort on the Oder, observed, that sparks taken from a polished body were stronger than those from a rough one. He could fire spirits easily with a polished conductor, but with difficulty, by means of one not polished.†

Ib. THE power of points to throw off the electric fire was communicated to Dr. Franklin by his friend Mr. Thomas Hopkinson, who electrified an iron ball of three or four inches diameter with a needle fastened to it, expecting to draw a stronger spark from the point, as from a kind of focus, but was surprised to find little or none.‡

174. THE manner in which Dr. Franklin first conceived the practicability of drawing lightning from the clouds, may be seen from an extract which he has given us from his memorandums. November 7th. 1749. After enumerating all the known points of resemblance between lightning and electricity, he concludes with saying, “ The electric fluid is attracted by
“ points. We do not know whether this property is in light-
“ ning, but since they agree in all the particulars wherein we
“ can already compare them, is it not probable that they agree
“ likewise in this? Let the experiment be made.”§

184. MR. WILCKE, in his remarks on Dr. Franklin’s letter, says, that on the 20th of August 1758, he saw his supposition verified; as he was viewing a large fringed cloud, strongly electrified, passing over a forest of tall fir trees. The

* Geschichte, Vol. 3. p. 528. † Ib. Vol. 2. p. 378. ‡ Dr. Franklin’s Letters, New Edition. p. 5. § Ib. p. 323.

ragged and depending parts of the large cloud were first attracted lower, and then suddenly rose higher, and joined the large cloud.*

184. HE was also an eye witness of two clouds lying one over the other, approaching, and flashing into one another. The lightning spread itself over all the parts of the blacker cloud, which was negative, and which immediately began to dissolve in rain.†

Ib. PROFESSOR WINTHROP observed his apparatus to be strongly electrified for several hours, while the snow, which fell the day before (and which had not electrified his apparatus while it was falling) was driven about by a high wind. This he had observed twice before.‡

207. SINCE the publication of the first edition of this work, Mr. Canton has hit upon another, much readier, and more powerful method of communicating electricity to the air than that described before. This he gives me leave to publish, and it appears to me to be of such a nature, as that it may very possibly lead to farther discoveries concerning the electricity of the atmosphere, and the phenomena depending upon it. “Take,” says he, “a charged phial in one hand, and a lighted candle, “insulated, in the other; and, going into any room, bring “the wire of the phial very near to the flame of the candle, and “hold it there about half a minute: then carry the phial and “candle out of the room, and return with the pith balls, “suspended, and held at arms length. The balls will begin “to separate on entering the room, and will stand an inch “and half, or two inches, apart, when brought near the “middle of it.”

* Page 351.

† Ib. p. 259.

‡ New Edition of Franklin's Letters, p. 434.

211. MR. LULLIN says, he produced much greater effects than those, of Dr. Beccaria's glass guns by making the electric spark visible in *oil* instead of *water*. Oil being a much worse conductor, the spark in it would be larger.*

240. To account for Mr. Delaval's experiments, Mr. Canton supposes, in a paper read to the Royal Society February 4, 1762, that stone, tobacco-pipes, wood, &c. will conduct when cold by the moisture they contain in that state; that where their moisture is evaporated by heat they become non-conductors; and that when they are made very hot, the hot air at, or near their surfaces will conduct, and the bodies will appear to be conductors again.

267. THE Abbé Nollet, in repeating the experiments of Mr. Symmer, found, that it was not absolutely necessary, that one of the stockings should be black; for that, if one of them was only dipped in a decoction of gall-nuts, which doth not dye them black, but is only a preparative to it, it would have the same effect.†

272. IN pursuance of the last mentioned experiments of M. Symmer, and another made at Pekin (which will be recited presently) S. Beccaria made the following, which are very curious; but which, after the example of the author, I shall relate without attempting an explanation.

HAVING charged a coated plate of glass, he slipped the coating from off the negative side, and applied another, uncoated and uncharged plate of glass close to it. After this, putting a coating upon the uncharged glass (so that the whole resembled one coated plate, consisting of two *lamine*) he formed a communication between the coatings. The consequence was an explosion, and a cohesion of the plates.

* Dissertatio Physica, p. 26. † Nollet's Lettres, Vol. 3. p. 42.

If he separated the plates before the explosion, after they had been in conjunction some time, the charged plate was positive on both sides, and the uncharged plate negative on both sides. But if he separated them after the explosion, the charged plate was negative on both sides, and the uncharged plate positive on both sides.

If after the explosion he separated and joined them again alternately, a small circle of paper, placed under the uncharged plate, adhered to it upon every separation, and was thrown off again upon every conjunction. This he could repeat even five hundred times, with once charging the plate. This was the experiment that he says was made by some Jesuits at Pekin, in the year 1755, and being sent to the academy of sciences at Petersburgh, was published in their memoirs, Vol. 8. p. 276.

If, in these experiments, the charged plate was inverted, and the positive side applied to the uncharged plate, all the effects were exactly the reverse of the former. If it was inverted ever so often, after remaining some time in contact with the uncharged glass, it would produce a change in the electricity. In the dark, a light was always seen upon the separation of these plates.

LAYING the two plates together like one plate, and coating the outsides of them, he charged them both together, and, at the distance of about four feet, he distinguished six of the *coloured rings*, which Newton describes in his book of Optics, all parallel to another, and nearly parallel to the edge of the coating. At the angles of the coatings the rings spread to a greater distance; where the coatings did not quite touch the glass, the rings bent inwards; and where the coatings adhered very close, they retired farther from them. Upon discharging those two plates, the coloured rings vanished, and the electric cohesion

cohesion (which Mr. Symmer had observed in this case) ceased with them.

UPON separating these plates before the explosion, that which had received the positive electricity was positive on both sides, and the other negative on both sides. If they were separated after the explosion, each of them (as in the former experiment) was affected in a manner just the reverse of this. Upon inverting these plates, that which was the thinner appeared to be possessed of the stronger electricity, and (like the charged plate in the former experiment) brought the other to correspond to it.

CHARGING the two plates separately, and taking off two of the coatings, so as to place the two positive or the two negative sides together, there was no cohesion or explosion. But joining a positive and a negative side, they cohered, and a communication being formed on the outsides, there was an explosion, which increased the cohesion. Making the abovementioned experiments with these plates, he says, they acted just as two that were charged at the same time.*

288. Mr. HARTMAN of Hanover has published an account of a curious experiment, which seems to show the progressive motion of the electrical explosion. He passes a shock through a great number of cannon balls, sometimes to the number of 40, placed near one another upon small drinking glasses; when all the sparks, he says, are seen at the same moment of time, and all the snappings make but one report; that when he substitutes eggs instead of balls of metal, the progress of the explosion is visible, every two giving a snap and a flash separately. This experiment requires weather very favourable to electricity, and, he says, has generally succeeded the best with only ten or twelve eggs.†

* Philosophical Transactions, Vol. 57. p. 458. † Abhandlung, p. 58, &c.

A VERY ingenious experiment has also been made by Amabeus Lullin of Geneva, in order to ascertain the direction of the electric fluid in explosions, and which he thinks comes nearer to the *experimentum crucis* than any other. He placed a common card in the circuit of the electrical explosion, while the wire which communicated with the positive side of the jar lay on one side of it, and that which communicated with the negative side lay on the other; their extremities not being placed opposite to one another, but at some distance. Things being thus circumstanced, he observed, that upon making the discharge, the card was constantly pierced close to the extremity of the wire which communicated with the negative side of the jar, as if the electric fluid, rushing from the positive of the jar into the wire which communicated with it, and issuing from the extremity of it, was driven by its own impulse along the surface of the card, and did not pierce it, till it came opposite to the extremity of the other wire, which communicated with the negative side, by which it was strongly attracted.*

294. Mr. WILCKE says, that, if a small piece of metal be hung in a silken string, opposite to a part of a glass jar, made thin for that purpose; upon the spontaneous discharge through that place, the piece of metal will be driven off to the distance of 5 or 6 inches.†

295. VIOLENTLY as the electric explosion generally affects the human body, we have accounts of some persons who could not be made to feel it; particularly three or four mentioned by Mr. Muschenbroek, among whom was a young woman.‡ I have also been told, that this was the case with a person near Leeds, who was at the same time a little paralytic.

* Dissertatio Physica, p. 24.

† Remarks on Franklin's Letters, p. 266.

‡ Monthly Review, October 1767, p. 250.

296. MR. LULLIN found, that a glass tube charged, and hermetically sealed, would show signs of electricity when it was heated.*

312. SIGNIOR BECCARIA produced a most beautiful appearance of electric light in the following manner. He conveyed positive electricity to a brass ball, suspended by a wire, within an exhausted receiver; when, upon presenting to it another ball in *vacuo*, the lower hemisphere of the former was most beautifully illuminated, with a visible electric atmosphere. When he conveyed negative electricity to the ball, the same beautiful illumination was observed on the ball presented to it. This experiment, he says, is a very delicate one, requiring great patience and dexterity, in adjusting the distances, &c. in order to make it succeed perfectly.†

313. I SHALL close this section of experiments and observations on electric light, with an account of a remarkable appearance which occurred to Mr. Hartman. When he had been making experiments four or five hours together, in a small room, and going out of it, and returned soon, with a lighted candle in his hand, walking pretty swiftly; he perceived a small flame to follow him, at the distance of about a step and a half, but it vanished when he stopped to examine it. He was a good deal alarmed at the appearance at first, but afterwards imagined it to be occasioned by the accension of the sulphur, which had been thrown into the air by the violent continued electrification.‡

351. I MUST introduce in this place a very curious experiment and observation of Mr. Lullin, concerning the production

* *Dissertatio Physica*, p. 32.

† *Phil. Transf.* Vol. 56. p. 107.
p. 135.

‡ *Abhandlung*,

of electricity in the clouds. He made a long insulated pole project from the side of one of the Alps; and, on the 29th of June 1766, observed, that when small clouds of vapour, raised by the heat of the sun, rose near the foot of the mountain, and ascended along the side of it; if they touched the extremity of the pole only, it was electrified; but if the whole pole, and consequently part of the hill on which it stood, was likewise involved, it was not electrified. From this he concludes, that the electricity of the clouds is produced by their passing through the air while the sun shines upon them. But to which of these two circumstances, namely, the motion through the air, or the action of the sun's rays this was owing, he could not determine, though he made several experiments for that purpose.*

379. MR. WILCKE relates a very remarkable appearance which occurred to himself, and which strongly confirms his supposition concerning the cause of hurricanes. On the 20th of July 1758, at three o'clock in the afternoon, he observed a great quantity of dust rising from the ground, and covering a field, and part of the town in which he then was. There was no wind, and the dust moved gently towards the East, where there appeared a great black cloud; which, when it was near his zenith, electrified his apparatus positively, and to as great a degree as ever he had observed it to be done by natural electricity. This cloud passed his zenith, and went gradually towards the West, the dust then following it, and continuing to rise higher and higher till it composed a thick pillar, in the form of a sugar-loaf, and at length seemed to be in contact with the cloud. At some distance from this, there came, in the same path, another great cloud, together with a

* *Dissertatio Physica*, p. 42.

long stream of smaller clouds, moving faster than the preceding. These clouds electrified his apparatus negatively; and when they came near the positive cloud, a flash of lightning was seen to dart through the cloud of dust, the positive cloud, the large negative cloud, and as far as the eye could distinguish, the whole train of smaller negative clouds which followed it. Upon this, the negative clouds spread very much, and dissolved in rain, and the air was presently clear of all the dust. The whole appearance lasted not above half an hour.*

391. MR. HARTMAN is of opinion that electricity is the cause of earthquakes, and gives a succinct enumeration of all the circumstances which favour this hypothesis.†

408. The first account I have met with of the application of electricity to medical purposes is of Mr. C. Kratzenstein, professor of medicine at Halle; who, in the year 1744, cured a woman of a contracted little finger in a quarter of an hour. He also so far relieved a person who had two lame fingers, by once electrifying them, that he could play upon the harpsichord, which he had before been disabled from doing. He also observed, that a man's pulse, which had beat 80 in a second before he was electrified, immediately after beat 88, and was presently increased to 96.‡

411. DR. EDWARD SPRY relates a complete cure which he made of a locked jaw and paralysis, in the case of a girl of 18 years of age. Small shocks were given to the muscles particularly affected.§

429. I MUST not omit to mention, in this chapter of Miscellaneous Experiments, what the Dutch writers have reported

* Remarks on Dr. Franklin's Letters, p. 348.

† Abhandlung, p. 148.

‡ Geschichte, Vol. 1. p. 294.

§ Phil. Transf. Vol. 57. p. 88.

concerning the gymnotus, a fish peculiar to Surinam, which very much resembles what naturalists relate concerning the torpedo. Mr. Muschenbroeck says, the gymnotus is possessed of a kind of natural electricity, but different from the common electricity, in that persons who touch it in water are shocked, and stunned by it, so as to be in danger of drowning. The fish has been taken, and put into a vessel; when experiments were made upon it at leisure; and it was found, that it might be touched with all safety with a stick of sealing wax; but if it was touched with a naked finger, or with a piece of metal, and especially a gold ring held in the fingers, the arm was shocked as high as the elbow. If it was touched with the foot, the sensation reached as high as the knee, and the pain was as great, as if the part had been struck with something hard. This kind of electricity is the same by night or day, when the wind is in every direction, when the fish was put in vessels of any materials, and whether it was in water, or out of water. Every part of the body of the fish is capable of giving this shock, but more especially the tail. The sensation is the strongest when the fish is in motion, and it is transmitted to a great distance; so that if persons in a ship happen to dip their fingers or feet in the sea, when the fish is swimming at the distance of fifteen feet from them, they are affected by it. Other fishes, put into the same vessel with it, presently die; but it is itself killed by the lobster. The gymnotus is found in the upper part of the river of Surinam, particularly the rocky part of it. It feeds upon all kinds of fish, and will even eat bread. This author proposes as a query, whether the sensation communicated by the torpedo does not depend upon a similar electricity; since Mr. Reaumur says, that when it is touched, the
hand,

hand, arm, and shoulder are seized with a sudden stupor, which lasts for some time, and is unlike any other sensation.*

THIS gymnotus, I suppose, is a different fish from the *anguille tremblante*, the *trembling eel*, which is also a native of Surinam, and lives in marshy places, from whence it cannot be drawn, except when it is intoxicated. It cannot be touched with the hand, or with a stick, without feeling a terrible stun, which reaches as high as the shoulder. If it be trod upon with shoes, the legs and thighs are affected in a similar manner. Fourteen persons joining hands, and the first of them touching it with a stick, they were all shocked violently. It is conjectured that this power of giving a shock resides in two muscles, which are particularly prominent and conspicuous.†

It is to be regretted, that none of the persons who have made experiments on these fishes, should have endeavoured to ascertain whether they were capable of exhibiting the phenomena of attraction and repulsion, or the appearance of electric light, as experiments of this kind are of principal consequence, and must have been very easy to make.

430. WITH respect to the power of points, it has been observed by Mr. Villette of Liege, that a needle, concealed in a glass tube, which projected an inch beyond it, took a stronger spark from a prime conductor than a man's finger; also that when the points of needles are covered with tallow, bees wax, sulphur, &c. they take peculiarly strong sparks. He adds, that when sulphur is used, and the sparks are taken obliquely, they are sometimes of a beautiful citron colour.‡

* Muschenbroeck's *Introductio ad Philosophiam Naturalem*, No. 901—909. † Philippe Fermin's *Natural History of Surinam*, p. 59. ‡ Nollet's *Letters*, Vol. 3. p. 212.

430. MR. LULLIN has made an additional observation to Mr. De Faye's, concerning the different manner in which conducting and non-conducting substances are affected, when they are exposed to the dew. He says, that if a plate of glass be extended upon silken threads, and exposed to the open air all night, and a plate of metal, less than the glass, be laid upon it, in the morning, the metal will be dry; and likewise the glass, on both sides, exactly under the metal; but that the edges of the glass, where the metal did not reach, will be wet on both sides.*

464. ACCORDING to Mr. Wilcke, bodies which have too great a proportion either of the electric fluid, or of their proper constituent matter, must avoid one another. In the former case, by the repulsion of the electric fluid, in the latter, by the repulsion of the constituent parts of the bodies.

488. Is not the electric matter the same with, or at least hath it not, some near relation to that luminous matter which forms the solar atmosphere, and produces the phenomenon called the *zodiacal-light*; which is thrown off principally, and to the greatest distance, from the equatorial parts of the sun, in consequence of his rotation on his axis, extending visibly in the form of a luminous pyramid, as far as the orbit of the earth, and which, according to Mr. De Mairan's ingenious, and at least, plausible hypothesis, falling into the upper regions of our atmosphere, is collected chiefly towards the polar parts of the earth, in consequence of the diurnal revolution, where it forms the aurora borealis?

Qu. May not the sun be the fountain of the electric fluid; and the zodiacal-light, the tails of comets, the aurora borealis,

* Dissertatio Physica, p. 54.

lightning, and artificial electricity, be its various, and not very dissimilar modifications?*

488. QU. Did not the sulphureous smell draw our attention towards the vitriolic acid, the peculiar hissing noise accompanying the electric blast, spontaneously issuing, for instance, from the pointed wire of a fully charged phial, appears rather to mimic the explosive action of deflagrating nitre; and may accordingly, without much violence, be supposed to arise from the nitrous aerial acid, violently commensuating with the phlogiston, which it either meets with in the air, or which is conveyed to it by the electrified body? Or were we to adopt the hypothesis of two distinct electrified fluids, we might, by way of temperament, propose as a query, whether the nitrous acid, &c. may not be the constant concomitant of those explosive pencils of light, which are observed to dart from the points of bodies replete with vitreous electricity; while the silent and languid luminous specks (resembling the small tip of a lighted match) appearing on the extremities of bodies endued with a resinous electricity, may as probably indicate the accension of a sulphureous matter, and consequently the presence of the vitriolic acid, the electric spark of explosion, appearing on the approach of the two bodies, being considered as the effect at least, in part of the menstrual action of these two acids on each other, &c.†

493. MR. WILCKE supposes, that when one plate of glass is charged by the very same electric matter which has been expelled in charging another plate, only part of that which is expelled from the former can enter the substance of the latter; because part must remain upon the wire, the coating, &c. and

* Monthly Review, October 1767, p. 353.

† Ib. December 1767, p. 458.

some must be lost. For the same reason, only part of that which is expelled from the second can enter the third, so that a very remote jar will not be charged at all.*

498. MR. WILCKE supposes the air to contract its electricity, in the same manner as sulphur and other substances do, when they are heated and cooled in contact with various bodies. Thus the air, being heated or cooled in the neighbourhood of the earth, gives electricity to the earth, or takes from it; and the electrified air, being conveyed upwards, by various means, communicates its electricity to them.†

Ib. SUPPOSING earthquakes to be caused by the discharge of a redundant electricity from the surface of the earth, might they not be prevented, in countries subject to them, by kites constantly flying very high, with wires in the strings, so as to promote an easy communication between the earth and the upper regions of the atmosphere?

The machine represented in plate the 8th, is constructed on the same general principles with the last; it is inferior to it in one respect, that it admits only of globes or cylinders with one neck, but these are far preferable to any other, and it is much more commodious for use, as it does not require any strong table like the other. It consists of a pillar of mahogany (*a*) standing upright on three feet, this pillar divides in two places to receive a wheel (*b*) in the lower part of it, and in the upper part a pulley (*c*), which is turned by a leathern strap (*d*), tightened by means of a small buckle; in the center of the pulley is a strong iron spindle, turning in two firm brass sockets fastened to each side of the pillar. In one of these sockets the extremity of the spindle turns upon a center, by means of a piece of iron (*e*) screwed

* Remarks on Dr. Franklin's Letters, p. 351.

† Ib. p. 302.

into it, while the other is held tight by a brass clasp, which may be made to hold it closer or more loosely at pleasure, by means of a screw (*f*). The iron spindle is made hollow in the form of a parallelopiped, in order to receive a piece of brass or iron, in which the brass cap that holds the globe (*g*) terminates. These are exactly fitted to one another, and by this means any globe may be taken out, and another put into the machine with very little trouble, if these parts be always made to the same patterns.—The rubber (*b*) is separated from the spring (*i*) by a plate of glass (*j*), which effectually insulates it, but the chain (*k*) connects them together, when positive electricity is wanted, as in the usual method of electrifying. The spring may be made to press more or less by means of a screw (*l*), and it may be raised higher or lower to suit globes of different sizes, by means of a contrivance which is not represented in the plate. The prime conductor (*m, n, o,*) is the same as in plate the 7th. From the same board which supports it arises another pillar, at the top of which is Mr. Lane's electrometer, the nob of which (*p*) may be placed as near to the nob opposite to it on the prime conductor (*q*) as is desired, by means of the graduated part (*r*). But the whole of this may be taken away when it is not wanted. When negative electricity is desired, the chain (*k*) must be removed from the rubber, and hung upon the prime conductor, so as to connect it with the table; and a short brass rod, with a nob at the end of it, must be screwed into a small socket, which will be found in the rubber above the plate of glass. This brass rod will then serve for a negative prime conductor; for in this situation, when the wheel of the machine is turned, this rod being insulated (together with the rubber through which all the electric fire passes to the globe) will receive sparks from whatever is presented to it, and therefore electrify negatively.

As it requires some dexterity and experience to turn the machine standing on three feet only without shaking it, small plates of brass, upon which the edges of heavy weights made of lead or iron may be placed, are fastened to two of them; but a large board may be firmly screwed under all the feet, or various other methods may be used, whereby the pillar, which supports the machine, may stand as firm as a person chooses.

522. MR. LANE's electrometer consists of a brass rod furnished with a knob, which, by means of a fine screw, may be fixed at any distance from the prime conductor, or any other fixed body communicating with the inside of a jar or battery. In consequence of this, the jar or battery, with which it is connected, can be charged no higher than the distance at which those fixed bodies are placed will permit, for at that height of the charge, the explosion will always be made between them.*

523. To make experiments with lightning in perfect safety, the electrified wire should be brought within a few inches of a conducting rod, that serves to guard the house, that the redundant electricity may pass that way, without striking any person that may happen to stand near it. The conductor to guard the house should consist of one rod, between one fourth and one half of an inch thick, if it be of iron, but smaller if it be brass or copper, terminating upwards in a sharp point, about four or five feet above the highest part of the building; and below, it should, if possible, be continued to some well or running water. Otherwise it should be sunk several feet into the ground, at the distance of some yards from the building. It is of no consequence whether this conducting rod be

* See M^r. Lane's more particular description of this useful instrument, Phil. Trans. Vol. 57. p. 451.

fastened

fastened on the inside, or outside of the house, or how many bendings are made in it.

524. MR. HARTMAN, in the construction of his apparatus for observing the electricity of the atmosphere with safety, makes use of long filken strings to support his metallic rod. These, therefore, require a large shed, fastened to the rod above them, to keep them dry. And lest the rain that falls upon this shed should carry off all the electricity, he makes a channel all round, which receives the rain; and thence he conveys it, under the shed, into an insulated receptacle.* But I cannot help thinking this complex apparatus unnecessary, especially if a solid stick of glass and small cover be used, instead of the filken strings and large shed.

536. IT has been the custom of many electricians to line their globes with sealing-wax, or some other electric substance, in order to make them act with more ease and vigour. M. C. L. Epinasse gives the following receipt for an electric composition for this purpose. Take 4lb. of venice turpentine, 1lb. of rosin and 1lb. of bees-wax. Boil these over a gentle fire, stirring them now and then, for 4 hours; at the end of which stir in a quarter of a pound of vermillion. Then a little of the mixture being taken out and left to cool, will be hard and brittle, a token that it is fit for use. Having well heated your globe or cylinder, pour the melted mixture into it, and turn it about, so as to spread it evenly over the inside surface, to the thickness of a sixpence, and let it cool very gradually.

. I make no doubt but that this electric lining is useful in some cases, as it was remarkably in two large globes of my own, the history of which I have given p. 585, but in the case of small globes, I cannot help thinking that Mr. L. Epinasse expects too much from it.†

* Anmerkungen, &c. p. 38.

† Phil. Trans. Vol. 57. p. 186.

546. MR. WILCKE was struck down senseless, by accidentally receiving, from his head to his feet, the charge of a large chemical receiver. He thought, that if he had received a shock five times as large, he should never have written the account of it.*

558. IF the electric sparks be taken from a brass ball, at the extremity of a long brass rod, inserted into the prime conductor, they will often be several inches long, and issue in a great variety of crooked directions, exactly resembling the course of lightning, and exhibiting a very amusing spectacle. A friend of Dr. Franklin's supposes, that the spark is thrown out of a strait course by the density of the air, increased by the action and re-action of the two fluids, which are repulsive of one another.†

559. THE Abbé Nollet has taken a great deal of pains to make the appearance of letters, and other figures, by means of electric sparks, and as it is impossible to make the sparks follow one another in a complete circle, on the same side of any flat surface, he makes use of plates of glass, and places one half of the circle, &c. on one side of the plate, and the other half on the other side, connecting the pieces that are nearest one another, but on different sides of the glass, by wires brought round it.‡ The description would be too tedious for this place, but the execution will be very easy, to any person who has but a little knowledge of electricity.

563. DR. FRANKLIN struck the six men down with two of his large jars (each containing about six gallons) not fully charged.§

* Remarks on Franklin's Letters, p 302. † Franklin's Letters, New Edition, p. 167.

‡ Lettres, Vol. 3. p, 281. § Franklin's Letters, New Edition, p 324.

A D D I T I O N S

T O T H E

ORIGINAL EXPERIMENTS.

601. **I** Have found that the supposition of mephitic air being a non-conductor was just. A charged phial may be dipped into a body of mephitic air, resting on the surface of a fermenting vat, without being discharged. If two equal phials, however, be equally charged, at the same time, and one of them be plunged into the mephitic air, and the other kept out of it; the latter will always retain the charge longer than the former, which will sometimes retain it but a very short time; owing, as I suppose, to the *moisture*, which is readily absorbed by the mephitic air. This circumstance accounts for the ambiguity of my former experiments. I, also, found *inflammable* air to be the same as common, or mephitic air, with respect to the power of conducting electricity.

616. OTHER persons, I find, have made experiments, which show how imperfect a conductor oil is. The following proofs of this are exceeding curious and pleasing. Mr. Cigna, Dr. of Medicine in Turin, observed electrical attraction and repulsion between conducting substances plunged in oil.*

* Nollet's Letters, Vol 3. p. 158.

MR. VILLETTE, optician at Liege, filled a dish of metal with oil, and when he had electrified the dish, he plunged a needle into the oil, and received a strong spark as soon as the point of it came within a small distance of the dish. A small cork ball being made to swim in this oil, upon the approach of the thicker end of the stalk of a lime, plunged to the bottom, and immediately rose again to the top.*

618. I FIND that Mr. Boze, very early, thought it was easy to distinguish the ores of metals from other earthy substances, by means of their greater conducting power.†

669. To account for the formation of the concentric circles, nothing seems to be necessary, but the supposition of the elasticity of the electric fluid, whereby its particles repel one another. For then, supposing a quantity of electric matter to issue from one piece of metal to another, through the air, it will endeavour to spread, but will be confined in its passage by the surrounding electric medium, and the strong attraction of the opposite metal. If this piece of metal have a flat surface, or one that is nearly so, the fluid will be attracted by it pretty equally, within a certain space; so that the mutual repulsion of its particles will have room to exert itself, and produce a division of the whole quantity; and as this repulsion is the same in all directions, the effect must be its throwing itself into a circle, or several concentric circles, on its entering the opposite piece of metal, and consequently melting it, in that form. For the same reason the circles themselves will consist of separate dots, each of which might have been caused by the fluid in another hollow circle, but being so small, the fusion of the metal could not show that circumstance.

* Nollet's Letters, Vol. 3. p. 312.

† Geschichte, Vol. 1. p. 293.

OF the circles being formed in this manner, I have been, in a manner an eye witness, when I have presented a flat piece of metal to a large prime conductor strongly electrified, and have seen large sparks, 5 or 6 inches long, divide about the middle, and strike the metal in a circle, about an inch in diameter ; generally with a central spot, but sometimes without one.

670. OTHER facts lead me to think, that all the instances in which the lightning has struck bodies alternately, may have been produced by a constant stream of electric matter, in a progressive motion, the first part of which being discharged upon any object, the stream is weakened when it meets with the second, but is accumulated again when the third comes in its way, and so alternately, as long as the stream and the line of objects continues. This account seems to be rendered more probable by the number of objects that have been struck in this manner, particularly in a case which is related by Dr. Wallace in his account of the Orkney islands, p. 78. “ In the “ year 1680,” he says, “ the lightning entered a gentleman’s “ cow-stall, where were twelve cows standing side by side, as “ they used to be, and killed every other one ; that is, it killed the first, and missed the second ; it killed the third, and “ missed the fourth ; and so of the rest, so that six were killed, and six remained alive and untouched.”

684. MR. CANTON has clearly proved the calcination of pure gold and silver by the heat of electrical explosion, producing numberless most beautiful globules of transparent glass, and also others tinged with all the varieties of colour from those metals. He has also made it probable, that the *black dust* mentioned in this section, is the calx, or glass of the metal, reduced to smaller particles than the laws of optics require to produce colour.

696. THE experiments which show that the electric matter prefers a passage along the surface of some imperfect conductors, seems to prove that the incumbent air is not in perfect contact with them, and that the passage through that *vacuum* is easier than through the substance of the conductor. For this reason, also, longer sparks may be taken along the surfaces of bodies that are non-conductors of electricity, as glass, baked wood, &c. than can be taken through the air. N. B. This remark, through inattention, is not inserted in the second edition of the history.

726. THE electric spark, taken in the middle of a phial filled with inflammable air, is always of a red or purple colour, and cannot be made to look white; but the larger the explosion is the nearer it approaches to white.

733. Mr. LULLIN also made several experiments with an electrified hollow vessel. He observed, that if an electrified cork ball, suspended by a silken string, was let down into the vessel, and touched the bottom, it left all its electricity behind it. He also made the experiment with a glass vessel, coated at the bottom, and charged, with the same event. It also made no difference whatever was the form, or size of the body let down into it, provided it was one third less than the depth of the vessel; but if, when it touched the bottom, it, at the same time, reached the top, or came near the top of the vessel, it acquired electricity; and a considerable degree, if it exceeded the top. The form of the vessel made no difference in his experiments, nor did it make any whether the vessel was entire or perforated. A wire net answered perfectly well. These experiments, Mr. Lullin imagines, clearly prove Nollet's doctrine

doctrine of the constant motion of electric atmospheres: For he thinks, that this free motion, on which electrification depends, is prevented from taking place within the vessel, by the contrary tendency of the opposite sides.*

733. DR. FRANKLIN thinks that, possibly, the mutual repulsion of the opposite sides of the *electrified cup* (see p. 709.) may prevent the accumulation of an electric atmosphere upon them, and occasion it to stand, chiefly, on the outside. But he recommends it to the farther examination of the curious.†

* Dissertatio Physica, p. 38. † New Edition of Franklin's Letters, p. 326.

SECTION XIII.

EXPERIMENTS IN WHICH RINGS, CONSISTING OF ALL THE PRISMATIC COLOURS, WERE MADE BY ELECTRICAL EXPLOSIONS ON THE SURFACES OF METALS.

IT was a discovery of Sir Isaac Newton, that the colours of bodies depend upon the thickness of the fine plates which compose their surfaces. He hath shown, that a change of the thickness of these plates occasions a change in the colour of the body, rays of a different colour being thereby disposed to be transmitted through it, and, consequently, rays of a different colour reflected at the same place, so as to present an image of a different colour to the eye. A variation in the density occasions a variation in the colour, but still a medium of any density will exhibit all the colours, according to the thickness of it. These observations he confirmed by experiments on plates of air, water, and glass. He likewise mentions the colours which arise on polished steel by heating it, as likewise on bell metal, and some other metalline substances, when melted and poured on the ground, where they may cool in the open air, and he ascribes them to the *scoria*, or vitrified parts of

of the metal, which he says most metals, when heated or melted, do continually protrude, and send out to their surfaces, covering them in the form of a thin glassy skin.

THIS great discovery concerning the colours of bodies, depending upon the thickness of the fine plates which compose their surfaces, of whatever density those plates may be, I have been so happy as to hit upon a method of illustrating and confirming by means of electrical explosions. A number of these being received on the surface of any piece of metal, change the colour of it to a considerable distance from the spot on which they were discharged, so that the whole circular space is divided into a number of concentric rings, each of which consists of all the prismatic colours, and perhaps as vivid as they can be produced in any method whatever.

It was not by any reasoning *a priori*, but by mere accident, that I was led to the discovery of these colours. Having occasion to take a great number of explosions, in order to ascertain the lateral force of them; I observed that a piece of brass, through which they were transmitted, was not only melted, and marked with a circle by a fusion round the central spot, but likewise tinged beyond the circular spot with a greenish colour, which I could not easily wipe out with my finger. Struck with this new appearance, I replaced the apparatus, and continued the explosions, till, examining with a microscope, I plainly perceived all the prismatic colours in the order of the rainbow. The diameter of the red, in this instance, was one-third of an inch, and of the purple near one-fourth. The diameter of the whole coloured space in the subsequent experiments, in which I generally used thirty or forty explosions, was near an inch.

PLEASED with the first experiment, I presently diversified it in a great variety of ways, the result of which I shall comprise in the following observations.

1. WHEN a pointed body is fixed opposite to a plain surface, the nearer it is placed, the sooner the colours appear, the closer do they succeed one another, and the less space they occupy. It seems, however, that when the point is at such a distance, that the electric matter has room to expand, and form as large a circular spot as the battery will admit, this coloured space is as large as it is capable of being made; but still the colours appear later, in proportion to the distance beyond that. When the point is fixed exceeding near, or made to touch the surface, the colours appear at the first explosion; but they spread very irregularly, and make no distinct rings.

2. THE more acutely pointed is the wire, from which the electric fire issues, or at which it enters, the greater is the number of rings. A blunt point makes the rings larger, but fewer. It is likewise much later before they make their appearance at a given distance.

3. IN making these rings, the first appearance is a dusky red about the edge of the central spot; presently after which (generally after four or five strokes) there appears a circular space, visible only in an oblique position to the light, and looking like a shade on the metal. This expands very little during the whole course of the explosions. It seems to be an attempt, as it were, at the first red, for, by degrees, as the other colours fill the greater part of that space, the extreme edge of it becomes a deeper brown.

4. AFTER a few more explosions, a second circular space is marked, by another shade beyond the first, being one-eighth
or

or one-tenth of an inch in breadth, which I have never observed to change its appearance, after ever so many explosions. This shade, by succeeding the first, which becomes gradually of a brown or light red colour, seems to be an attempt at the fainter colours, which intervene between the reds.

5. All the colours make their first appearance about the edges of the circular spot. More explosions make them expand towards the extremity of the space first marked out, while others succeed in their place; till, after thirty or forty explosions, three distinct rings appear, each consisting of all the colours. If the explosions be continued farther, the colours become less beautiful and distinct, the red generally prevailing, and suffusing the rest.

6. THE innermost, *i. e.* the last formed colours, are always the most vivid, and those rings are likewise closer to one another than the rest.

7. THESE colours may be brushed with a feather, or a finger may be drawn over them, without injury to them; but they are easily peeled off with one's nail, or any thing that is sharp. The innermost are the most difficult to erase.

8. THE first rings are sometimes covered with a quantity of black dust, part of which, however, may be wiped off with a feather, and the colours will appear under it.

9. IT makes no difference whether the electric matter issue from the pointed body upon the plate, or from the plate upon the pointed body, the surface opposite to the point being marked exactly alike in both cases; also the points themselves, from which the fire issues, or at which it enters, are coloured to a considerable degree, about half an inch. The colours, also, return here as upon the plate.

10. THE

10. THE more circles are made at the same time, the more delicate, I think, the colours will be, whereas the surface is torn, as it were, by violent explosions, and the colours appear rough and coarse. But this roughness is only perceived on steel. On silver, tin, and polished brass, the colours were always equally free from that coarseness.

11. A POLISHED surface is not necessary for these colours, for they appear very well, though they do not make so beautiful an appearance on the rough surfaces.

12. THESE coloured rings appear equally well on all the metals that I have tried, viz. gold, silver, copper, brass, iron, steel, lead, and tin. I have not tried any of the semi-metals, but I have no doubt of their answering as well as the proper metals.

13. WHEN the pointed wire was made to incline to the plane on which the colours were made, the circular spot was quite round, and the centre of it in a perpendicular let fall from the point upon the plain surfaces; but the colours were projected in an oblong form, the centre being in the pointed wire continued.

UPON shewing these coloured rings to Mr. Canton, I was agreeably surprized to find, that he had likewise produced all the prismatic colours from all the metals, but by a different operation. He extended fine wires, of all the metals, along the surface of pieces of glass, ivory, wood, &c. and when the wire was exploded, he always found them tinged with all the colours. They are not disposed in so regular and beautiful a manner as in the rings I produced, but they equally demonstrate, that none of the metals, thus exploded, discovers the least preference to one colour more than to another. A variety of
of

of other very extraordinary appearances occurred in the course of Mr. Canton's experiments in melting wires, but I forbear to mention them, as I hope he will soon favour the public with a communication of them himself.

IN what manner these colours are formed, it may not be easy to conjecture. In Mr. Canton's method of producing them, the metal, or the calcined and vitrified parts of it, seem to be dispersed in all directions from the place of explosion in the form of spheres, of a very great variety of sizes, tinged with all the variety of colours, and some of them smaller than can be distinctly seen by any magnifier. In my method of making these colours, they seem to be produced in a manner similar to the production of colours, on steel, and other metals by heat, *i. e.* the surface is affected, without the parts of it being removed from their places, certain plates or *laminæ* being formed, of a thickness proper to exhibit the respective colours, at certain distances; and that the thickness of these plates is continually changing by the repetition of the explosions.

N. B. The battery made use of in the above-mentioned experiments, was of twenty-one square feet of coated glass.

SECTION XIV.

EXPERIMENTS ON THE LATERAL FORCE OF
ELECTRICAL EXPLOSIONS.

BEING informed, in accounts of damages done by lightning, of persons and things being removed to a considerable distance, without receiving any hurt; I was excited to try whether I could produce similar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power, but I do not know that this effect has ever been so much as taken notice of by any electricians. The experiments I presently found to be very easy, and I think it not difficult to ascertain the cause, and the manner in which this striking effect is produced.

IF pieces of corks, powder of any kind, or any light bodies whatever, be placed near the explosion of a jar or battery, they will not fail to be moved out of their places, upon the instant of the discharge. If the explosion of a large battery be made to pass over the surface of animal or vegetable substances, in the manner described above, and large corks be strewed along or near the path intended for it, it is surprizing to observe with what violence they will be driven about the room.

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This dispersion is in all directions from the centre of the explosion, and it makes no difference whether the rods, between which it is made, be sharp pointed or otherwise.

THE effect of this lateral force is very remarkable in attempts to fire gunpowder in electrical explosions. If the gunpowder be confined ever so close in quills or cartridges, and they be held fast in vices ; yet, when the explosion is made in the centre of them, it will sometimes happen (even when a wire has been melted in the midst of the powder, and the fragments have been seen red hot, for some time, in different parts of the room) that the powder has not been fired, or only a few grains of it, the rest being dispersed with great violence, part of it flying against the faces of persons who assisted in making the experiments. This circumstance, together with the charcoal being a conductor of electricity, makes it so extremely difficult to fire gunpowder by electrical explosions ; and it is evidently owing to this lateral force, that parts of the melted wire fly so many ways, and to so great a distance from the place of explosion.

THIS lateral force is exerted, not only in the neighbourhood of an explosion, when it is made between pieces of metal in the open air, but also when it is transmitted through wires that are not thick enough to conduct it perfectly ; and the smaller the wire, and the more complete the fusion, the greater is the dispersion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the explosion (in which case it generally assumes a dusky red, which lasts but for a moment) there was a small dispersion from every part of the wire, but by no means so great, as it would have been if it had been melted, or only heated to a greater degree.

By a considerable number of trials, I found, that a greater force of explosion would move light bodies at a greater distance, but the smaller the bodies were, the less was this difference; so that I imagined, that if they had no weight at all, they would, probably, be moved at the same distance, by the explosion from any quantity of coated surface, charged equally high: but there was a great difference in the weights removed by different forces at the same distance. Placing the same piece of cork at the same distance from the place of explosion, I found that the discharge of one jar removed it one-fourth of an inch, two jars one inch and one-fourth, three jars one inch and three-fourths, and four jars about two inches; so that I do not wonder at very heavy bodies being moved from their places, and to considerable distances, by strong flashes of lightning.

THAT the immediate cause of this dispersion of bodies in the neighbourhood of electrical explosions, is not their being suddenly charged with electric matter, and therefore flying from others that are equally charged with it is, I think, evident from the following experiments and observations. I never observed the least sensible attraction of these light bodies to the brass rods through which the explosion passed, or to the electric matter passing between them, previous to this repulsion, though I used several methods, which could not have failed to show it, if there had been any such thing. Sometimes I suspended them in fine silken strings, and observed that they had contracted no electricity after they had been agitated in the manner described above. Sometimes I dipped them in turpentine, and observed that no part of it was found sticking, either to the brass rods themselves, or to any part of the table betwixt them and the place where the light bodies had been laid.

laid. I even found that the explosion of a battery, made ever so near to a brass rod, did not so much as disturb the equilibrium of the electric fluid in the body itself; for when I had insulated the rod, and hung a pair of pith balls on the end opposite to that near which the explosion passed, I found that the balls were not in the least moved at the time of explosion; which they would have been, if part of the electric fluid natural to the body had been driven, though but for a moment, towards the opposite end. I also observed that the effect was the same when the explosion was made to pass through one of the knobs of the insulated rod. This lateral force was evident through thin substances of various kinds, interposed between the explosion and the bodies removed by it; as paper, tinfoil, and even glass; for when some grains of gunpowder were put into a thin phial close stopped, and held near the explosion of a battery, they were thrown into manifest agitation.

I THEREFORE think it most probable, that this lateral force is produced by the expulsion of the air from the place where the explosion is made. For the electric matter makes a vacuum of air in its passage; and this air, being displaced suddenly, gives a concussion to all the bodies that happen to be near it. Hence the removal of the light bodies, and the agitation communicated to the thin substances, and to the air, and the light bodies placed beyond them.

THE only objection to this hypothesis is, that this lateral force is not so much less in vacuo as might be expected, when the air is supposed to receive the concussion first, and to communicate it to other bodies; but it must be considered, that the most perfect vacuum we can make with a pump is not free from air. I have tried to make this experiment in a torricel-

lian vacuum, but could not succeed at that time. Besides, as the electric matter of which an explosion consists must take a wider path in vacuo, if not equally fill the whole space, it may affect a body in its passage, without the intervention of any air. In condensed air, this lateral force was not, as far as I could perceive, much increased.

WILLING to *feel* what kind of an impulse it was that acted upon bodies, when they were driven away by this lateral force of electricity, I held my finger near the path of an explosion of the battery, passing over the surface of a green leaf, when I felt a stroke, as of something pushing against my finger. Several corks, placed in the same situation, were driven to a considerable distance by the same explosion.

RECOLLECTING that this power, which I now call the *lateral force of electric explosions*, must be the same with that which gives the concussion to water, mentioned in my experiments to imitate an earthquake, and to vegetable and animal substances, over the surface of which it passes ; and being determined to make a more satisfactory trial of it than I had ventured to do before, I laid a green leaf upon the palm of my hand, intending to make the explosion pass over the leaf ; but the leaf was burst, and torn to pieces, and the explosion passing over my hand gave it a violent jar, the effect of which remained in a kind of tingling for some time.

LASTLY, In order to judge the most perfectly of this force, I laid a chain, communicating with the outside of the battery, upon my naked arm above the wrist, and bringing the discharging rod near the flesh, within about two inches and an half of the chain, I made the explosion pass over that quantity of the surface of the skin. Had I taken a greater distance, I was aware that the explosion would have entered the flesh ; which,
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I was sensible, would have given a painful convulsion to the muscles through which it passed. In this case, the sensible effect was very different from that, being the same external *concussion* as before; and I have sometimes thought, that the sensation is not disagreeable. However the hairs upon the skin were all singed, and curled up along the whole path of the explosion, and for the space of about half an inch on each side of it; also the *papillæ pyramidales* of the skin were raised, as when a person is shivering with cold. This was also the case in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the explosion, and the place on which the chain had lain, had a redness, which remained till the next day. Sometimes the flesh has contracted a blackness by this experiment, which has remained for a few hours.

SECTION XV.

VARIOUS EXPERIMENTS ON THE FORCE OF ELECTRICAL EXPLOSIONS.

MAKING the explosion of a battery pass over the surface of a green cabbage leaf, I observed that it left a track near a one-fourth of an inch in breadth, exceedingly well defined, and distinguishable by a difference of colour from the rest of the leaf. Along this path also the firmness of texture in the leaf was entirely destroyed, that part becoming quite flexible, like a piece of cloth. Presently after it turned yellow, grew withered, and became perfectly brittle.

WILLING to try the effect of this explosion passing along the surface of other substances, I laid a piece of common window-glass on the path, pressed by a weight of six ounces; but it was shattered to pieces, and totally dispersed, together with the leaf on which it lay. Placing the black side of a piece of cork-wood upon it, pressed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it, about half an inch in breadth, and a quarter of an inch in depth. Laying the smooth cut surface of the piece of cork, it was furrowed all the way as if it had been

been cut with a file, but not near so deep as before. Many of the small pieces, which had been rubbed off in the explosion, remained in the furrow. Also the substance of the cork seemed to be shattered, and it was easily rubbed off a little way into it.

I MADE this explosion on the surface of some red wine in a small dish, and kept a part of the same quantity exposed in a similar manner; but I could perceive no difference between them after several days.

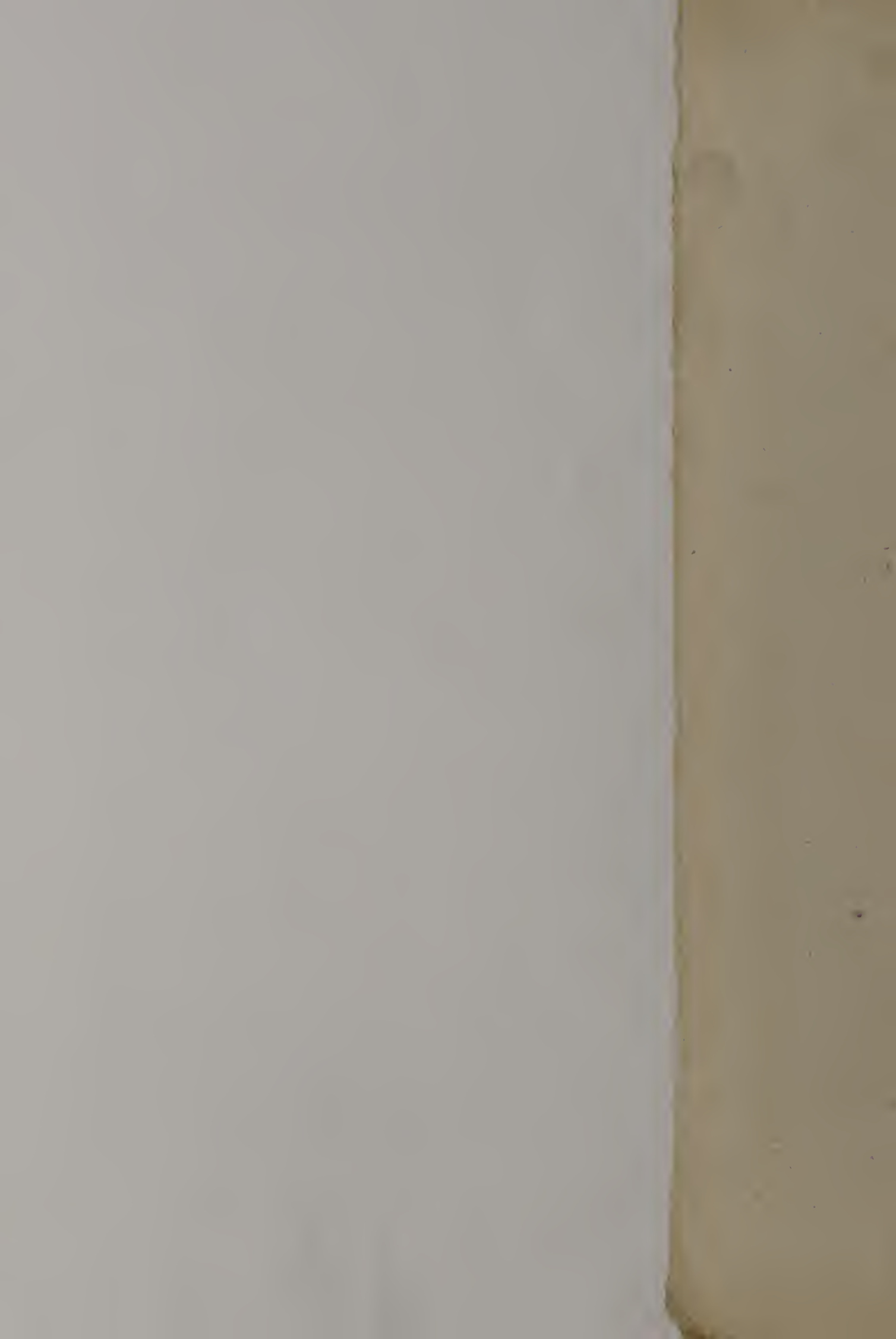
THE track of an electric explosion on the surface of the cabbage leaf being so well defined, suggested an experiment to ascertain whether there was any sensible *momentum* in the electric fluid, when it is rushing with violence from one side of a battery to the other. For this purpose I made the explosion pass over the leaves when they were cut in right and acute angles; so that the shortest path, from the inside to the outside of the battery, was to turn close at the angle; and observed, that it was not diverted from its course in the least degree by the rapidity of its own motion, but that it had turned exactly at the angle, and kept as close to the opposite side, as if the motion had begun at the angle. The electric matter had however been evidently attracted by the veins of the cabbage leaf, having pursued them a little way, at least, having sensibly affected them wherever it met with them in its passage.

THIS experiment suggested another, intended to determine whether the force of an explosion was at all diminished by being diverted from a right lined course, and made to turn in a great number of angles. To do this, I first found, by a great number of trials, what length of a small iron wire I was able to melt with a battery of about twenty square feet, in the middle of a circuit of about three yards of brass wire, considerably

derably thicker than the iron, and stretched in two right lines, suspended on silken strings. The length of the iron wire melted in these circumstances was about three inches. I then took the same brass wire, and, fixing pins into a board of baked wood, twisted it about them, making it turn in a very great number of acute angles; and I put three inches of the same iron wire in the middle of this crooked circuit, that I had done in the straight one, so that the electric matter in the explosion was obliged to make a great number of turns at acute angles, before it could come to the iron wire; but I always found that the same length of iron wire was melted in these circumstances, as in the other, and not the least difference was perceived in the force.

BUT though the *form* of the wire through which an explosion passed made no difference in its force, I found a very remarkable difference occasioned by the *length of the circuit*, in wires of the same thickness, and which, I own, surprized me very much.

IN order to ascertain the practicability of firing mines by electric explosions, I took twenty two yards of small brass wire (but so thick, however, that I could not have melted the least part of it, by the force of any battery I have ever constructed) and extending it along a dry boarded floor, with a small piece of iron wire, and a cartridge of gunpowder about it, in the place that was most remote from the battery; I found that, upon the discharge, the wire was not melted, nor the gunpowder exploded; also the report was very faint. In other circumstances a charge of the same battery was able to melt more than nine inches of this iron wire, and this same cartridge was easily fired near the battery, connected with shorter pieces
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of the same brass wire ; so that the diminution of force must have been owing to the length of the circuit.

IN the place of this small brass wire, I substituted an iron wire, one fifth of an inch thick ; when about half an inch of the small iron wire was exploded ; so that the force was not lessened so much in a circuit of the thick iron wire, as it had been in one of the small brass wires. In order to judge how much of the force might be lost by nearer circuits, consisting of less perfect conductors, I joined the middle of the circuit made by the iron wire with water, in which both the wires were immersed. The effect was, that the small iron wire was only made red-hot, but not exploded as before.

BEING sensible how much depended upon avoiding all lesser circuits, whereby part of the fire of an explosion might return to the battery, without reaching the extremity of the circuit, where I intended the whole of its force to be exerted ; in the remaining experiments, I insulated half the circuit of iron wire. There was no occasion for insulating the whole circuit : for if there was but one passage *to* or *from* the middle of it, there could be but one *from* or *to* it. In this method it was easy to ascertain what loss of the force was occasioned by the length of the circuit, as every other circumstance was carefully excluded. And it presently appeared to be very considerable ; for though I could melt nine inches of the small iron wire at the distance of 15 yards from the battery ; when I tried twenty yards, I found that I was just able to make six inches of it red-hot. The battery in these experiments was in the house, and the wires of which the circuit consisted were conveyed by silken strings into a garden adjoining to the house.

MENTIONING this loss of force, occasioned by the length of the circuit in electrical explosions, to Dr. Franklin, he told me, that the same observation had occurred to him, and that he also had been disappointed in an attempt to fire gunpowder at a distance from his battery.

STRUCK with this appearance, I endeavoured to ascertain the quantity of this obstruction, by trying what other courses the electric fire would chuse, preferably to a long metallic circuit. In the first place, taking about a yard of the small brass wire mentioned above, I disposed it in the manner described Fig. 9. Pl. 1. connecting one of the ends with the outside of the battery, and the other with the inside. In the first place I brought the parts *a* and *b* (near the two extremities) into contact, and, upon the discharge, found there had been a fusion in that place, and that a great part of the fire had taken the shorter circuit, though it had been obliged to quit the wire in one place, and enter it again in another. Afterwards I removed the parts *a* and *b* to a small distance from one another, and, upon the explosion, observed a strong spark pass between them. Removing them to greater and greater distances, I found the explosion chose to pass above one-third of an inch in the air, rather than make the circuit of the continued wire. Using a longer and smaller iron wire, the passage through the air exceeded half an inch. I then took four or five yards of iron wire, one-tenth of an inch thick; when the passage through the air was still half an inch; and taking three yards and a half of a wire that was one-fifth of an inch thick, the spark in the air was half an inch, and sometimes near three quarters of an inch. Making use of only half the length of this wire, the passage through the air was only half that distance or one-fourth of an inch. When I kept the place of near contact about the
middle

middle of this wire, and made the explosion at the extremities of the whole wire, I was obliged to bring them about as near again, *i. e.* to little more than one-eighth of an inch before the passage would be through the air; so that the force of the whole explosion must have been greatly weakened by its passing through so much of the wire. Lastly, I took a pair of kitchen tongs, the legs of which were two feet in length, and the smallest part of them above half an inch in diameter; when the circuit was made about one sixth of an inch in the air (for at that distance from one another the ends of the tongs had been fixed) rather than through four feet of that thick iron.

NOTWITHSTANDING this passage of the explosion through the air, at the same time that a metallic circuit was open for it, it was evident that the whole of the force did not pass this way, nor indeed the greatest part of it. For when I extended a small iron wire between *a* and *b*, I could only make about half an inch of it red-hot, whereas, when there was no other metallic circuit, I was able, with the same battery, to explode more than two inches of it.

As the electric fire meets with so much obstruction in passing through a circuit of iron of this thickness, I make no doubt, but that it is considerably obstructed in passing through metallic circuits of any thickness whatever; and that it would prefer a passage through the air, if they were made even of no great length.

IN this method, the different degrees of conducting power in different metals may be tried, using metallic circuits of the same length and thickness, and observing the difference of the passage through the air in each. N. B. A common jar answers as well in these experiments as a large battery.

IT is evident, from many experiments, that the whole fire of an explosion does not pass in the shortest and best circuit, but that if inferior circuits be open, part will pass in them at the same time. Of this I made the following satisfactory trial. I took an iron chain, and laid it upon a table, in contact with a charged jar; so that the parts of it made two circuits for the discharge, which I could vary at pleasure; and I observed that, when one of the circuits was but half an inch, and the other more than half a yard; yet, if the charge was high, it always went in them both, there being considerable flashes between the links of the remotest part of the chain. If the charge was weak, it passed in the shorter circuit only.

IT is evident that when the wires of a battery are not in close contact, there must be some loss of force in the discharge; but this never appeared to me to be so considerable, as Mr. L'Epinaffe seems to have imagined. *Phil. Tran.* vol. 57. part 1. p. 186. In order to ascertain this by experiment, I first found, by repeated trials, what length of an iron wire, of a certain thickness, I was able to melt with a battery consisting of twenty jars constructed in the manner described above. It was about two inches and a half. I then soldered all the wires together, and also soldered one rod to them all, instead of a chain which I had used before; so that I avoided near a hundred sparks, in each of which some force had been lost; but I did not find, after many trials, that the power of the battery was sensibly diminished. I still could not melt three inches of the same wire.

F I N I S.

